With the Author's
compliments

A

CRITICAL ANALYSIS

OF

THE MEMOIR

READ BY DR. BARRY BEFORE THE ACADEMY OF SCIENCES,

ON THE 8th OF JUNE, 1825, AT THE INSTITUTE OF FRANCE,

ON

ATMOSPHERIC PRESSURE

BEING

THE PRINCIPAL CAUSE

OF THE

PROGRESSION OF THE BLOOD IN THE VEINS.

BY HENRY SEARLE, SURGEON.

LONDON:

CALLOW AND WILSON,

16, PRINCES STREET, SOHO.

1827.

Printed by J. Davy, Queen Street, Seven Dials.

TO

SIR ANTONY CARLISLE,

PROFESSOR OF ANATOMY,

&c. &c. &c.

THIS SHORT ANALYSIS

OF

DR. BARRY'S RECENT MEMOIR

IS RESPECTFULLY DEDICATED

BY

HIS MUCH OBLIGED FRIEND,

AND MOST OBEDIENT HUMBLE SERVANT,

HENRY SEARLE.

21, *Charlotte Street, Bloomsbury,*
Feb. 10, 1827.

PREFACE.

THE work which Dr. Barry has lately produced, upon the Venous Circulation, as affected by atmospheric pressure, has caused, as might be expected, considerable excitement amongst the members of his profession; and feeling on my own part, perhaps more than a common interest in the subject, I shall endeavour to prove the fallacy of the theory therein contained, and doubt not, that it will soon be followed by abler and more experienced professional writers.

The preface of Dr. Barry's book, will not, I should imagine, be perused without calling forth sentiments unfavourable to the author; however, every friend to science must respect, and even recognise the motives, which alone could have given rise to the marked liberality and attention which Dr. Barry received from certain eminent persons of the faculty in France, in aid of prosecuting his enquiries, connected with the motion of the blood in the veins, and which Dr. Barry very justly acknowledges. But

in the next place, he makes those allusion to a subject, which, in my humble opinion, demand a reply, for in the subsequent quotations Dr. Barry says:—

"The letter* of" the "Director of the Veterinary School at Alforts, will shew with what noble zeal the science of physiology is cultivated in France. In England, on the contrary, an outcry has been raised of late, not entirely unsupported by some leading professional men, against every thing like enquiry, having for its basis direct experiment upon living animals; yet, the little that we know of the laws of life, is drawn from this source alone. The examination of a quiescent machine, can only suggest the use of its parts, when they were all in movement. Well directed experiments upon these same parts, actively employed in fulfilling their various functions, either confirms the suggestion, giving it the validity of a law, or at once destroys the whole fabric of a baseless theory.

* * * * * * * * *

"They who inveigh most loudly against experiments upon living animals, and who affect an

* Referring to a Letter published in the Appendix of Dr. Barry's book.

excess of sensibility, have never made any experiments themselves; they are contented with the exposition of what they, in their wisdom, suppose nature OUGHT to do, instead of investigating what she actually does.

"Others talk of needless cruelty. If any useful knowledge is to be obtained by an experiment, none of the means necessary to arrive at this knowledge, can be needless, and none else can be adopted without defeating the purpose aimed at; therefore, in useful experiments, there never is needless cruelty, or, in other words, unnecessary pain inflicted."

In answer to these bold assertions, I think I am warranted in stating, that there are but few persons who would exclaim against experiments upon living animals, which had the decidedly useful tendency of discovering any important law in physiology. It is to the honor of the medical profession IN ENGLAND, that some of its members have complained of the appalling violation of humanity, in those numerous instances where experiments have been made, merely from the motive of curiosity, without any view of advancing science. IN FRANCE, ON THE CONTRARY, the wanton cruelty which is there practised, is too notorious. Although Dr. Barry cannot possibly be implicated in this charge; yet, judging

from his own experiments, it may be concluded, that his notions of "well directed experiments," are not very correct, and therefore, that he comes forward with a very ill grace to defend the physiologists of France, or of any other nation, from the impeachment of exercising "needless cruelty." Such remarks are truly unfortunate, since Dr. Barry admits that some of the most distinguished men in Paris, actually witnessed his experiments, and expressed a satisfaction of their success, when they ought to have seen the rationale of them, and rejected them as "baseless" to his theory.

It may be here observed, that it is not surprising, that many of the profession not feeling sufficient interest in the subject, to induce them to investigate it thoroughly, should have implicitly lent their credence, after it had obtained such high sanction. It certainly is a very prevalent error, to receive as undeniable proofs, all successful experiments, that is, when the ends for which they were made are accomplished; it should be recollected, that an experiment, of whatever kind, is a problem founded upon certain data; and in its *progress* involves a number of collateral circumstances, not apparent to the eye, and but to the understanding of those who are already acquainted with the laws of nature under which they are influenced, and that these

together offer a train of reasoning which comprehends the *entire* operation of the experiment, and with certainty unfolds the truth; while the bare evidence of ones senses affords such a *partial* view of its performance, that it never ought to be relied upon.

These remarks are peculiarly referable to Dr. Barry's experiments, by which he has made so many proselytes to his theory, while there is not one which will bear the least examination; they are merely proofs that there has been "unnecessary pain inflicted."

Dr. Barry finishes his preface in the following manner:—

"One word as to the essays and experiments, a vague unauthenticated notion that the return of the black blood to the heart is, in some undefined way, influenced by suction, may be traced as far back as the time of Harvey. Haller, and many others also, noticed a marked coincidence between the respiratory movements of the thorax in the warm-blood mammalia, and the motion of their venous blood.

"But the mechanism was never pointed out, by which nature, in these animals, applies the MIGHTY AGENCY of atmospheric pressure to the veins, and connects, as cause and effect, the expansion of the chest with the afflux of the centripetal fluids to the heart. *The experiments,*

therefore, that demonstrate this mechanism, and supply these important desiderata in physiology, must be entitled to the meed of novelty, along with whatever other merits they may possess.

"The first experiment" (says Dr. Barry) "however nearly it might have been approached, was never made; that upon the pericardium,—*was never even imagined by any man living or dead, before me.* Whether my conclusions be just or true, must be soon decided—in the mean time, as the most intense power of the reasoning faculties of man, can never arrive at a discovery so perfectly original, as to be entirely unconnected with every thing that was known or suggested before, I shall reply to those who deny the originality of my researches in the words of the great Haller.

"Præterea æquo animo oportet expendisse, non eum verum inventorem esse, cui vaga aliqua cogitatio elapsa est, in multo fundata experimento, sed eum omnius eam laudem mereri, qui verum et suis fontibus, per sua pericula, suas que meditationes, cruerit, et adeo firmis rationibus stabiliverit, ut veri cupidos convincerit."—

Haller, tom. i. lib. 3.

Amongst the manifold efforts of men, in the different departments of science, one finds an

endless variety of talent and character mingled together. It has been figuratively observed, that some will even attempt to set the world on fire, and in the book under my notice, one is strangely informed, that this potent blaze can be kindled in Paris without much fuel. Dr. Barry has made considerable inquiry whether his theory has ever occurred to the minds of any other persons. I can in veracity assert, that it did present itself to me about four years ago, and most probably it has to others: but he was not likely to find in print any thing so palpably discordant with some of the plainest phenomena in the animated part of the creation. As soon as this wonderful discovery was to be seen reported in the newspapers, which was a few months prior to the appearance of Dr. Barry's book, several friends, knowing that I was devoting particular attention to the circulation of the blood, apprized me of it; to whom the replies made, were sufficient to convince them that I was already familiar with the hypothesis, and that I viewed it as nothing more than an ephemera. However, the ground which it has gained, shows that it ought to be refuted, since every error which becomes current, is a serious misguide in the way to truth, and particularly as Dr. Barry has promised further to *enlighten* the profession with a second Memoir, explanatory

of the objections raised concerning fishes, and of other facts equally availing.

Therefore, unless some check be given, it is not improbable, judging from the nature of his first productions, that he may offer us an extension of his theory, and assert that he has also been making (what he might please to call) an Experiment, to prove our credulity, and that he has established the fact of there being a "relative vacuum," within the sculls of some of the faculty, formed by the absence of several important organs of the brain, and that the continuance of life is no disproof, as the "mighty agency of atmospheric pressure" determines the blood to the cavity, and supports the other parts of the cerebrum: for Dr. Barry has openly said, in the presence of a number of persons, and within my own hearing at the Hunterian Society, that it is not fair to judge of his theory by his Book, and that if we would suspend our objections until the appearance of his second Memoir in the Spring, that we should be perfectly satisfied of its truth, which seemed to be a sad libel upon his own delusive system. As the following hypothesis has been raised upon physical grounds, it has been thought best to contest it more particularly upon the same.

A

CRITICAL ANALYSIS

OF

DR. BARRY'S MEMOIR

ON

THE MOTION OF THE BLOOD IN THE VEINS.

In order to enter fairly into a critical analysis of the theory contained in this Memoir by Dr. Barry, it becomes, in the first place, requisite to obtain well defined notions of the powers which are presumed to propel the blood through the veins, and of the relation they bear to each other.

" The object of the following Memoir," (says Dr. Barry, in page 1) " is to demonstrate, by proofs, drawn from the anatomical structure of animals, and from direct experiment,

" First.—The powers by which the blood is propelled through the veins to the heart.

" Secondly.—The comparative velocity with which it is moved through the veins, and through the arteries.

" Thirdly.—That the constant supply of blood to the heart, cannot depend solely upon the causes to which it has been hitherto ascribed."

Dr. Barry commences, in page 2, by an enquiry " What is the amount of all that has been hitherto proved, relative to the circulation of the blood?"

In allusion to Harvey's idea, that the heart was the sole power engaged in this function, he says:

" Later physiologists have done but little to show either the truth or the error of Harvey's assertions. They have merely admitted a few secondary sources of impulse to the blood; such as,—1. The contractile power of the arteries, whether the effect of muscular or elastic fibres. 2. The insensible contraction of the capillaries, supposed to be independent of the heart. 3. The action of the veins themselves upon their contents. 4. The pressure of muscles of voluntary and involuntary motion.

" Of these supposed powers, some are so little

susceptible of being demonstrated by direct experiment, others must be so uncertain in their operation, and the theories which they have been brought to support are so opposed to each other, that the evidence against is, a priori, nearly as strong as that in favor of their existence."

Hence it may be seen, that Dr. Barry estimates these four auxiliary powers as of very inferior importance; but in the following quotations, from pages 56 and 57, he enumerates those which he deems the principal, and on which he builds his theory.

" From what we have seen in Dr. Barry's Memoir, and from what has been said in the supplement, it is evident that fluids, whether moving through living or through inert tubes, obey the laws of pressure, and of gravitation; and that in the quiescent living animal, the only demonstrable active powers employed by nature to propel the contents of the veins towards the heart, are—

" First.—The impulse given by the pressure of the heart itself, continued through and propagated by the arteries. By this power, the

blood is sent into the situation where it can be most favourably acted upon, by

" Secondly.—Atmospheric pressure, diminished or entirely taken off, around the cardiac ends of the venous tubes, during the expansion of the chest; but unaltered and entire around every other part of their surface, opposed only by the gravity of the fluid acted upon.

" Thirdly.—Gravitation, when the heart is relatively the most depending point, or when this power is acting with the pressure of the heart's contraction upon the base of the venous column."

Here then are specified by Dr. Barry, three powers essential to the motion of the blood in the veins, viz. the vis a tergo, atmospheric pressure, and gravitation. Of these, the last is evidently the least, as the heart is seldom, to any considerable extent, the most depending part; while it is more commonly opposed to the major part of the venous circulation. The vis a tergo appears to be placed second in importance. It is allowed to exert its influence throughout the arterial system, and it may be inferred to the smaller

veins, as this is the situation alluded to where it is supposed that the blood can be most favorably acted upon by atmospheric pressure, and where the accumulation is said to occur: arguing the termination of this power, as may be seen in his fourth conclusion, at page 36, "As the blood passes through the greater veins during inspiration only, whilst it is incessantly traversing the arteries, it follows, that an accumulation must take place somewhere, between these two orders of vessels." Atmospheric pressure, as the title of Dr. Barry's book implies, will be found the grand power, which is supposed to be employed in circulating the blood through the veins; for in reverting to page 57, Dr. Barry says:

"Of these powers, the pressure of the atmosphere is by far the most intense in its degree, the most constant in its influence, and the most unvarying in its amount. It is that, without which, the circulation could not be maintained beyond a few moments."

Also, in his conclusions at page 35: "From what has been said, and from what has been

observed in the experiments, the two following facts may be considered as PROVED:—

" First.—That the cavities of the great veins within the thorax, and all the thoracic cavities, draw towards them the fluids with which they are placed in direct communication.

" Second.—That this attraction, or suction, never takes place but during the expansion of the thorax, that is, during inspiration.

" From these facts, and from what we have seen in the last experiment, we may conclude,—

" 1st.—That the blood which runs contrary to its own gravity, arrives at the heart only during *inspiration.*

" 2dly.—That the power which impels it at this moment through the veins, is *atmospheric pressure.*"

In these quotations from Dr. Barry's book, it is unequivocally admitted, that atmospheric pressure is actively applied, even to the whole of the venous system, exteriorly to the thorax, excepting, of course, that part within the skull;

and his numerous experiments upon absorption also maintain this.

It would not have become necessary to be thus particular in settling these points, had any degree of Candour been shown on certain occasions; however, it will enable the reader the better to judge, in the progress of this review, of the degree of evidence which the experiments are pretended to afford—of the validity of the premises upon which this theory is founded.

In the next place, it is proper to give, at considerable length, the anatomical arguments which Dr. Barry brings forward, as they are somewhat peculiar, as well as essential to his experiments.

" It seemed to me," says Dr. Barry, in page 5, " impossible that the alternate expansion and contraction of the thoracic cavities, should not affect the contents of the great veins opening into them, in the same manner as the expansion of a pair of bellows *would the contents of flexible tubes*, in communication with their cavities. I reasoned thus :—

" The right and left cavities of the thorax have within them, each a lung or bag, divided

into a greater or lesser number of distensible cells, communicating with one another, and, with a common tube, the Trachea. When the chest is enlarged by the act of INSPIRATION, air rushes in through this tube, to distend the air-cells, and force them to occupy that space, in which the expanding parieties of the thorax tend to leave a vacuum. But as it is evident that the air would follow the expanding sides of the chest, much more readily if there were no cells to be distended; and as it is *an unalterable law*, that all liquids in communication with an enlarging cavity will be pressed towards it, if exposed at the same time to atmospheric influence, it became presumable that blood would be forced into the thorax through the cavæ, during inspiration.

" Having once caught this view of the part which respiration might probably bear in the circulation, particularly of the venous blood, several known facts presented themselves in support of its correctness, viz. the swelling of the external jugular veins during *expiration*, and their immediate collapse during *inspiration*. The checking of certain hœmorrhages by forced in-

spirations; the fatal accidents that have been known to follow the opening or the dividing large veins; and, above all, the situation of the heart itself, placed in the centre of the chest in a bag, at all times too large for its volume, and which seems not only protected from direct atmospheric pressure, but is probably enlarged in all its diameters by the act of inspiration.

" Upon turning my attention more particularly to the anatomy of the thoracic viscera, I was struck with the analogy which I thought was observable between the mechanism of the heart, pericardium, and mediastinal pleuræ, as resembling a pair of bellows, and that of either lung within its proper cavity, compared to the same instrument.

" The situation of the fibrous bag of the pericardium, in the human subject, and the covering which its lateral surfaces receive from the mediastinal pleuræ, reflected over them from the roots of the lungs behind, and from the sternum and ribs before, are well known to all anatomists. When the lungs are expanded, their surface is necessarily enlarged. When the ribs carry for-

ward the sternum, and when the diaphragm presses down the abdominal viscera, the internal surface of the thorax is also enlarged; consequently, the pluræ covering these surfaces is put upon the stretch, and that portion covering the pericardium on either side, is pulled upon at its margins, on both sides, in the directions best calculated not only to protect the fibrous bag from pressure, but to enlarge its cavity throughout. The motion of the sternum during inspiration, tends to bring the anterior surface of the pericardium forwards and upwards. The synchronous movement of the diaphragm, tends to enlarge it downwards, and to complete the analogy. As each lung is furnished with a pipe, through which it receives and discharges air, so is the heart, with its receiving pipes (the veins), and its discharging pipes (the arteries), through which it receives and discharges blood.

" But as the aorta, the great discharging pipe of the heart, is equally employed during both stages of respiration, in sending blood out of the thorax, it seemed probable (if my reasoning with regard to the effect of inspiration upon the

blood of the cavæ were well founded,) that enough of blood should be brought into the chest during its expansion alone, to supply the discharging tubes during a whole act of respiration. Thus the necessity of a reservoir became evident, into which this blood might be drawn by the expansion of the three thoracic bellows.

" Having by these arguments, and others now unnecessary to be recapitulated, brought my hypotheses thus far, I came to the following presumptive conclusions.

" 1.—That a liquid, such as water, in an open vessel, being by means of a tube placed in direct communication with the cavity of one of the great veins within the thorax of a living animal, would be forced, by atmospheric pressure, to rise in the tube, and that the motion of the liquid within the tube, would be regulated by the respiratory movements of the animal.

" 2.—That the same phenomena would be exhibited by establishing the same communication between the liquid, and any of the cavities around the vein."

The first Experiment by Dr. Barry.

"Having first ascertained upon the dead horse, that a tube of proper size and length might be readily introduced down the jugular vien, as far as the anterior cava, I proceeded thus—I selected a horse condemned to be destroyed on account of an incurably diseased hoof, but sound in every other respect. The animal having been thrown upon his right side, I laid bare his left jugular vein, tied it below its middle, and about an inch below the ligature introduced into its cavity, in a direction towards the heart, a large-sized flexible catheter, having a spiral glass tube fitted into its outer end; the rounded point of the catheter was cut off above the lateral openings.

* * * * * * * *

"When the horse was thrown, his breathing became almost entirely thoracic; the rising and falling of his ribs could be readily and distinctly counted; the respiration was also audible. The catheter having been pushed towards the heart

* A reference is here made to the instrument used, which need not be transcribed.

as far as it would go, a ligature, which had been passed under the vein a little below the opening made to admit the catheter, was firmly knotted round both.

"The point of the spiral tube, over which I had hitherto held my finger, was now immersed in a cup of water, deeply coloured by a solution of common Prussian blue. The moment that I removed my finger, the blue liquid rose through the spiral, and flowed rapidly towards the heart. The sun happening at the moment to shine strongly on the tube, I saw, in the most satisfactory manner, the undissolved particles of blue pass up from the cup, and round the spiral, during *inspiration*, and halt, or return slowly towards the cup, during expiration. Not a drop of blood was seen to enter the tube, but bubbles of air sometimes appeared upon the surface of the liquid in the cup during expiration. The breathing being audible, allowed me to keep my eye steadily fixed upon the motion of the liquid, and to ascertain, beyond all possibility of deception, that this motion was entirely dependent upon the movements of respiration."

* * * * * * * *

"To vary the proofs of this *wonderful* coincidence between the movements of the blue liquid in the tube, and the respiration of the animal, I withdrew the" tube "from the liquid in the cup for a moment, during inspiration, so as to admit one or two bubbles of air, and returned it again immediately." A space more or less extensive of the tube became thus transparent. Upon the next inspiration these bubbles were forced round the spiral with considerable velocity, and the whole tube again became uniformly blue, by the ascent of more liquid from the cup. This part of the experiment, several times repeated, invariably afforded the same results.

"A considerable quantity of cold water, and also of air, had now been forced into the vein, and thence to the heart. The animal gave strong indications of suffering, and as the fact, that inspiration produces a relative vacuum within or around the anterior cava, was considered as fully established, the experiment was discontinued. I FORGOT TO MENTION, that towards the latter part of the experiment, when the animal's respiration became hurried and irregular, blood ap-

peared in the tube, on two or three occasions, during expiration. The next inspiration, however, invariably restored the blue liquid to its place. During the trials and repetitions of this experiment which I made upon horses, I had occasion to remark :—

" 1.—That when the animal was standing, although the coloured liquid invariably rose in the tube, atmospheric pressure was never so distinctly marked as when he was prostrate. This I proved by experimenting upon the same animal in both positions.

" 2.—That the connexion between the motions of the liquid in the tube, and the respirations, cannot be satisfactorily observed while the horse is standing, because his breathing, when in the erect posture, and at rest, is scarcely, if at all perceptible.

" 3.—That when the respiration became hurried, from whatever cause, or when it was embarrassed by disease, there was frequent regurgitation of blood through the tube ; but never once did this occur, except at the moment of expiration ; and never, under any circumstance,

did the liquid ascend the tube, except at the moment of inspiration; this experiment, repeated upon the anterior and posterior cavæ of dogs, afforded similar results."

This first experiment of Dr. Barry amply verifies some of the observations in my preface. It appeared simple, and afforded that partial evidence, of merely being seen, that its results were precisely what the experiment was *made* to evince; when, upon examination, the only legitimate deduction from it is, that under certain circumstances, a vacuum tending to be formed within the thorax during inspiration, will allow any fluid (as water) influenced by atmospheric pressure, to pass through an *incompressible* tube, communicating with that cavity, to supply its place; but that the blood will rush through the veins under ordinary circumstances, to prevent the formation of this vacuum, is not in the least degree proved; on the contrary, a comparison of the data upon which this experiment is founded, with those pertaining to the phenomenon of nature, which it is pretended to imitate, forcibly shews that the

operation of one cannot be expected either to resemble, or to explain the function of the other. For instance, the catheter made use of, although flexible, was incompressible; so was the spiral glass tube, its appendage; yet this apparatus, dissimilar, and even opposed as it were in its property to a vein, was thrust down through it into the thorax, to become a substitute. But in order to prove that both cannot be subservient to the same purpose, the following Experiments are related.

A portion of the jugular vein, one inch in length, was affixed to two inches of metallic pipe of about the same calibre, making together a tube of three inches long; its compressible third was immersed in a glassful of water, taking care to keep it patulous, that the fluid might ascend to a level with that external to it in the vessel; I then applied my mouth to the metallic end, in order to try and suck the water up out of the glass, but without success; for the atmospheric pressure upon the water, instead of forcing it up through the tube against its own gravity, collapsed the sides of the vein contiguous to the in-

compressible part of the cylinder, effectually precluding any of the liquid from ascending to my mouth: shewing that veins, from their compressibility, are incapable of being the media through which atmospheric pressure might affect the vacuum forming in the thorax during inspiration.

Another Experiment was made after the manner of Dr. Barry's, upon the jugular vein of a Horse, yet differing in using a catheter, without a spiral glass tube being attached, and in not pushing it down the vein, "as far as it would go," but merely connecting the instrument with it, by introducing it an inch, and placing a ligature around both. The effect of this was completely at variance with the result of Dr. Barry's Experiment, no water could ascend a tube which was not compressible throughout, for the very reason just assigned, that atmospheric pressure would collapse the jugular vein with much less resistance, than was met with in raising the water against its gravity.

Similar Experiments to the last, have been performed upon Horses by Mr. Ellerby, a surgeon

residing in the city, without being aware that I was pursuing the same enquiry. The results were exactly the same: On one occasion, he passed the tube down the vein within an inch of the cavity of the chest; and this slight difference, between his and Dr. Barry's Experiments, was sufficient to oppose the theory in question: for the fluid *did not pass up into the chest*, confirming what has been endeavoured to be pointed out, viz. that compressible tubes are not at all fitted to become the media of communication between atmospheric pressure, and a vacuum.

Another important variation in the Data of the first experiment by Dr. Barry, is in the circumstance of throwing the horse down; or of any other incautious mode of performing it, that would in the least agitate the animal. It is well known that the immediate effects of fear, upon any warm-blooded animal, are hurried and deeper respirations. To shew how materially this circumstance affects the validity of Dr. Barry's proofs, the following Experiments will serve.

Anticipating the result, I laid bare the jugular vein of a Horse while standing, taking great care

not to disturb his respiration. After introducing the catheter into the vein as far down as it would go, and securing it as directed, on immersing the exterior end into water, contained in a graduated glass, *not a drop* rose up within the tube. The animal was then thrown down upon his side, when his respiration became difficult and quickened, and on trial the water immediately ascended in a considerable quantity at each inspiration.

A somewhat similar Experiment was repeated: but in its second stage, instead of throwing the animal down, a blow was given to him on the side, a practice among Horse Dealers to try the wind; and which decidedly distinguished the difference in the effects of a natural, and a hurried respiration.

The Rationale of these experiments evidently is, that while respiration is moderately slow, the calibre of the windpipe is suited to the varying capacity of the thorax; and will allow the air, influenced by atmospheric pressure, to rush down in a sufficient volume, to enable the lungs to keep pace in their expansion, with that of the chest; but, that when respiration becomes quick

and voluminous, by the inordinate action of the muscles engaged, the natural and healthy relation just alluded to, is so far destroyed, that the air will not rush through the trachea, with a velocity greater than in the former instance, by the sum of the difference resulting from the altered circumstances: Consequently, even water, whose inertia exceeds eight hundred and twenty times that of air, is forced through an incompressible tube, in aid of supplying the more rapid tendency to the formation of a vacuum.

By way of further illustration of these Experiments, and to support the reasoning already applied to them, the following description is given, of the construction and operations of a pair of bellows made for the purpose. It differs from the one in common use, in having a pane of glass in its ceiling; and instead of a valve in its floor, a tin well sunk, with a cork plug for an occasional outlet; in having also three tin pipes to pass through its sides, in a manner not to interfere with its action exteriorly, and these are bent down at a right angle, that they may be readily plunged into as many vessels of water; the nozle is as usual, and represents the trachea,

although by much too small; the tubes, Dr. Barry's veins; the capacity of the bellows, that of an ordinary sized human thorax. When this apparatus is made to imitate respiration by slow movements, air, whose specific gravity is to water as one and two-ninths to a thousand, takes a vast precedence, and rushes through the nozle at each elevation of the handle with due velocity, and supplies the increasing space without the assistance of the heavier fluid; thus resembling the Experiment upon the horse *standing*, while his breathing was undisturbed by agitation; but when the artificial respirations of the bellows are augmented beyond a given number in a minute, a small quantity of the water may be seen through the window, to drop from the pipes into the well: to a similar extent would an animal, under one of Dr. Barry's operations, draw in water, who was but slightly agitated; however, when the movements of the apparatus are greatly accelerated, at each expansion, a considerable quantity of the fluid is transferred from the glasses without, to the well within the bellows, corresponding to what was observed when the animals were prostrate.

It is to be understood, that it is not contended that the atmospheric pressure is so limited in its powers, that it could not force air down the nozle in the last instance, with a velocity proportionately greater than was found necessary in the first; the only inference maintained, is that less exertion of those powers was required under these circumstances, to supply the vacuum forming with both fluids conjointly, than would have been exercised, in giving adequate increase to the velocity of the motion of a fluid so elastic as air: and as a proof of this, if mercury be placed in the glasses, in lieu of water, on dilating the bellows, it will not ascend the tubes; therefore the rapidity of the air's influx must become duly increased.

The bellows were constructed under the mistaken notion, that air would rush into the thorax, to supply a vacuum, without being sensibly resisted by the lungs, whose cells are of such extreme delicacy, and placed, as it were, in the centre of that vacuum; however, upon reflection, I am convinced that Dr. Barry has, very properly, made a distinction in this respect: nevertheless, it is presumed, that he has inferred too much in doing so.

The experiments with the bellows are useful, in showing the marked difference in the influx of fluids of various densities: they also physically explain, why a horse, breathing naturally, will not draw a liquid of the specific gravity of water, into his thorax; while, under difficulty and hurried respirations, he will; and although the free space of the bellows does not accord with the obstructed cavity of the chest, there are other circumstances which afford more than an equivalent for this disparity; viz. the capacity of the bellows is about equal to a human thorax, while its nozle is not a sixth part of the size of a trachea, which would correspond with it; and at each expansion, it enlarges its cavity several times more than would occur in animal respiration.

Dr. Barry has not given any definite idea of the resistance which the lungs, in their collapsed state, offer to air rushing in to distend them, at the dilatation of the thorax; but concludes that it is nearly equal to that which the venous blood would occasion, in being forced into that enlarging cavity.

It would be difficult to ascertain the amount of

this impediment, from the complexity of circumstances which the subject involves; however, the following mode is suggested, as affording a more satisfactory answer than might, a priori, be expected to the question, which, by the bye, is to be understood of a comparative nature, and had better be placed in that form: viz. What would be the degree of force opposed to *atmospheric pressure,* in impelling air into the lungs, to distend them during the enlargement of the thorax at the time of inspiration, *in relation* to that which would be opposed to *it,* in determining the blood in the veins of all parts of the body connected with the right auricle of the heart, (excepting that within the cranium,) to occupy the same increasing space.

In directing our enquiries, in the first place, to the former part of the question, let it be assumed, that the inertia of the lungs, charged as they may be with blood, is one power to be overcome—their weight being about a pound and a half. Another is, their resilient disposition. A third is, multiplied ramifications of the bronchial tubes, in dividing the air, during its influx, into as many columns.

In the second place, in reference to the vacuum being supplied by blood. The resisting powers to the atmospheric pressure, are,—First, the inertia of more than three-quarters of the whole mass of venous blood, circulating throughout the frame, and which is probably not less than a pound and a half in weight. Secondly, the inertia and elasticity of nearly all the intervening substance, between the external surface of the body, and the cavities of the veins alluded to. And Lastly, the friction, from the blood passing through the vast multiplicity of venous tubes; and which, on comparison, is evidently greater than that from the air rushing through the ramifications of the bronchia.

Again,—the Second Power, just enumerated, so far exceeds the corresponding one, the resiliency of the lungs, that they cannot be compared together, unless as a mole hill would be to a mountain. However, as the relation of these forces cannot be very satisfactorily determined, we will pass over them, for the sake of conciseness, and bring the two first powers of resistance to form the definitive position of this question: Thus, as

the inertia of the lungs is stated to be about equal to that of the given mass of venous blood, if both advance to supply the vacuum forming within the thorax during inspiration, what will be the difference in the velocity of their respective movements? Its solution will give the momentum required for each purpose, which, of course, will imply the relative degrees of resistance overcome: at the same time, it will perhaps enable the reader to form a more correct notion, than is in general entertained, respecting the distension of the lungs whilst performing their function, which may prove some relief from the prolixity of this part of the enquiry. In pursuance, let it be supposed, that a human being, of middle stature, is in the recumbent position. That the mean length of one horizontal axis of the cavity of the chest, measuring from the arches of the ribs, on one side to those on the opposite, be eleven inches; that the mean length of the other, in the direction from the diaphragm, towards the cervical vertebræ, is also eleven inches; and that the vertical axis, from the sternum to the vertebral ends of the ribs, be eight inches:

making the area of the thorax, of these dimensions, about sixty-nine cubic inches. If firm pressure were to be made upon the abdomen, towards the diaphragm, by the hand of another person, so as to prevent this muscle acting, while natural respirations were maintained; and if, at the same time, a piece of tape were placed around the trunk, over either of the lower six ribs, it would be found, at inspiration, that the ends of the tape would not recede from each other more than half an inch; and, upon admeasurement over the upper ribs, there would be still less enlargement in the circumference, so that the mean increase in the diameter, would not be one-sixth of an inch: and however trivial this degree of expansion assigned to the thorax might appear, one-sixth would actually exceed the reality; for it would augment its capacity sixteen fluid ounces, while but few people commonly inhale a pint of air at each time; the motion of the lungs, during their distension, must accordingly be very inconsiderable indeed. For instance, as the vertebræ form the mesial line, and divide the principal lobes, either cannot become

extended in the direction from its base towards its thin edge, more than one-twelfth of an inch: but if the entire lung were moved forward equally, it is clear that it would not become distended, but merely displaced, the base then must remain stationary, while its distant edge were moved one-twelfth of an inch; so that if the inertia of the whole lung be considered to be overcome during inspiration, its mean motion must be attributed to it, which is half the two extremes: one being as nothing, and the other one-twelfth, making one-twenty-fourth of an inch. But since all the larger bronchial tubes are unyielding, being cartilaginous, it is most probable that a great proportion of the lungs are not disturbed, particularly towards the base, where the principal larger blood vessels are to be found. Assuming that this calculation be nearly correct, it can readily be perceived, that the lighter and more delicate parts of the lungs can be moved one-twelfth of an inch in any two directions, perpendicular to each other, during their usual distension.

For the sake of argument, suppose that the

whole mass of the lungs, with their blood, are moved in the same manner, one-twenty-fourth of an inch, and this be taken as the amount of the resistance offered to the atmospheric pressure, when forcing air into the thorax, to prevent the vacuum forming there during its expansion; it only remains to be determined, the distance the continuous columns of blood must advance in the veins, to occupy the same space in the chest. One of Dr. Barry's premises is, that the venous blood enters the chest only during inspiration. If, then, there be twelve respirations in a minute, while there are seventy pulsations of the heart and arteries, their proportion to each other will be nearly as six to one. Supposing that there are five pulsations to one inhalation, and that the heart projects one ounce of fluid, at each systole, into the vascular system, it follows that five ounces of blood must return to the heart at each inspiration; but it would be found, that this quantity would fill at least ten inches of the venæ cavæ: the inference then is, that the whole mass of venous blood, stated to be involved in this question, advances ten inches, while the lungs only

advance one-twenty-fourth of an inch, making a difference of two hundred and thirty-nine in the extent, and therefore in the velocity of their motion: consequently, if the inertia of the lungs be a pound and a half, and their velocity be taken as unity, the momentum will be equal to a pound and a half: while the inertia of the blood being the same, and its velocity two hundred and forty, the momentum will be three hundred and sixty; and the *relation* in the powers of resistance offered to the atmospheric pressure, in determining air through the lungs, and blood through the veins, to supply the same vacuum, will of course be as one to two hundred and forty. Shewing how little Dr. Barry can attribute to this impediment of the lungs, and, at the same time, suggesting the nature of the dispnœa which attends inflammation, and other diseases of the lungs, whereby their weight becomes increased; and, from the little movement required during respiration, it is probable, that adhesions do not take any part in the production of this difficulty.

The Experiments and Arguments which have already been mentioned, in disproof of the vali-

dity of the basis which Dr. Barry's first Experiment offers to his theory, are equally opposed to his second Experiment; which need not be transcribed, as it differs merely in inserting several catheters into the general cavity of the thorax, between its parieties and the lungs, instead of passing one down within the jugular vein; that indeed was decisive, but it only proved that a liquid like water can be drawn through *incompressible* tubes, under the former *peculiar* circumstances. A dog was the subject of the Experiment.

The great importance which Dr. Barry attaches to his Experiments upon the pericardium, has been noticed in the preface: The annexed Experiment is one of those he refers to.

Third Experiment of Dr. Barry.

" A similar communication still remained to be established, with the bag of the pericardium: but hitherto, in all the trials which I had made upon the dog, the cavities of the heart had been penetrated, and the result of the Experiments thus rendered inexact. The long and delicate

connexion between the pericardium and sternum in this animal, added much to the other difficulties. The pericardium of the Horse, I found to be the most favorably circumstanced for my Experiments. In this animal, it is attached to the periosteum of the upper surface of the sternum, from the fourth rib backwards, extending its adhesion posteriorly to the base of the xiphoid cartilage, from whence it turns sharply upwards and forwards, behind the heart, to be attached to the lower surface of the posterior pulmonary veins. By dissecting up the point of the xiphoid, I was able to pass a pointed tube along its upper surface, through the lower margin of the diaphragm, and into the pericardium, at its posterior and inferior angle, without penetrating the peritoneum. The tube was armed with a caoutchouc bag, as in the last* Experiment; through this bag, I passed a flexible catheter into the tube, nearly to its point. Thus, when the pericardium was penetrated, the catheter could be pushed in immediately, and to any length, so as to

* Dr. Barry's Second Experiment.

prevent the heart from being wounded, by beating against the point of the tube.

" In all the cases in which I succeeded in establishing a communication between the bag of the pericardium exclusively, and a coloured liquid, the fluid rose in the tube as rapidly as in the former Experiments, and in all but one, its motion upwards was governed by the animal's inspirations. In all, however, with the exception of this single case, although the liquid invariably halted or descended during expiration, there was an oscillation of the fluid upwards, which seemed independent of respiration, but could not be observed during inspiration, because then it was confounded with the general motion of the liquid upwards.

* * * * * * * * *

" When either of the ventricles was penetrated, an accident which frequently happened, as long as the blood was allowed to flow through the tube, the animal did not seem likely to perish sooner than he would have done by any other hæmorrhage of the same amount; but when the effusion took place within the pericardium, he

invariably died when the bag was filled to its utmost extent. In these cases, the heart was found compressed, and smaller than natural, in the midst of an immense coagulum."

Here then, is an admitted proof, either that a vacuum is not always formed around the heart, or that it was destroyed: the egress of the blood sufficiently affirms that there was none, while the circulation was not influenced in any way, although *deprived of this indispensable agent*. Dr. Barry's surprising candor must be repeated—"The animal did not seem likely to perish sooner than he would have done by any other hæmorrhage of the same amount."

Again, in page 30, in the same tenour, Dr. Barry says, "In support of the importance of the pericardium, in the mechanism of the circulation, it may be remarked, that it is perhaps the only part of the animal which is never found entirely wanting."

Although this may be a fact in Anatomy, its weight is very inconsiderable in Physiology, since

it is the cavity of the pericardium which is the *sine qua non* contended for; and which, not unfrequently, becomes entirely obliterated in disease; of this there are many instances on record, and some preparations may be met with in museums. Mr. Langstaff has an excellent specimen of the kind, which evinces decided traits of a very chronic character, proving that this cavity is not absolutely essential to life, nor, consequently, to the circulation of the blood. Hydrops pericardii is another pathological example, in disproof of the necessity of a vacuum being formed arouud the heart; for the pericardium commonly assumes a figure somewhat globular, and when filled to a degree of tension, it must approach still more to that form: therefore, says Dr. Barry, "When the ribs carry forward the sternum" (which, by the bye, seldom exceeds one-sixth of an inch), "and when the diaphragm presses down the abdominal viscera," instead of enlarging the area of this bag, by pulling it in nearly opposite directions, and thus giving a tendency to form a vacuum within it, it must (if any effect of the kind be produced) diminish the space, so as to

compress the heart.—A result the very contrary to that upon which this part of the hypothesis is based; for it is mathematically evident, that a hollow globe cannot sustain any change in its form, without having its cavity decreased.

But in order to obtain an unequivocal proof, that the circulation of the blood is perfectly independent of the aid of a vacuum forming around the heart, an experiment was made, which probably could not have been more simple and apparent, nor less imitative. I removed sufficient of the parieties of the thorax, over the region of the heart of a fully grown rabbit, to enable me to introduce a finger, and raise the heart upon its base, and give any direction to its long axis; here was an influx of atmospheric pressure directly around this organ, where the vacuum ought to occur; the impossibility of its occurrence need not be urged. This animal bled profusely from the wounded extremities of two or three intercostal arteries, which exhausted it for a time; it afterwards ate, ran about, and lived seven hours; when it was killed, to prevent any suffering taking place, from the inflammation which would have supervened.

It may be suggested, that, from the little inconvenience manifested by this Experiment, an operation might be performed upon the human frame, in severe cases of hydrops pericardii; by trephining a small hole through the left side of the sternum, and introducing a fine curve-pointed syringe, to withdraw the water.

Having now gone through the examination of that part of Dr. Barry's theory connected with the right side of the heart; the pulmonary venous circulation, pertaining to the left side of that organ, becomes the next object of inquiry; for which purpose, it will be found proper to make another quotation, in full, of Dr. Barry's anatomical inferences; there being so many circumstances peculiarly considered, that extracts merely could not well be made, without mutilating the subject.

Pulmonary Venous Circulation.

" Before I state" (says Dr. Barry, at page 22) " the inferences which appear to me deducible from the facts already recorded, I shall say a few words on the motion of the blood in the veins of the lungs.

"Since it is evident that the blood sent into the aorta, cannot arrive through any other channel than the pulmonary veins, it will not be unreasonable to conclude, either that the lungs must be equally pervious to the blood of the right heart during all the stages of respiration; or, that if they are not so, there must be a reservoir from which the left heart can be supplied during the period when they are least pervious.

"The lungs themselves are placed within two cavities, which, as we have just seen, are in a state of tendency towards the formation of a vacuum during the act of inspiration, and therefore the pulmonary veins would, at first sight, appear to be all equally exempt from pressure in every part of the thorax, at the moment of its expansion. A more attentive examination, however, will shew, that nature has ensured, by a beautiful and simple mechanism, as constant and as ample a supply to the left heart, as she has to the right, and by the same means; viz. atmospheric pressure. I shall take the thorax of the horse, as an example to illustrate the pulmonary venous circulation in the warm-blooded mammalia.

"In the horse, the posterior cava quits the spine, as soon as it arrives at the crus of the diaphragm; it then runs along this muscle for a considerable distance, until it arrives opposite the base of the heart, when it passes into the thorax like a rope across a room, unconnected with every thing for five or six inches of its length, except with the thin, gauze-like membrane, which extends from the right side of the pericardium to the diaphragm, and which seems to hang from the outer and upper side of the thoracic cava, like a curtain. As this membrane conducts the phrenic nerve to its destination, I shall take the liberty of calling it the phrenic curtain, not being aware of any other name by which it may be distinguished.

"The two great posterior, or right and left pulmonary veins, form, by their early confluence in the right cavity of the thorax, behind the pericardium, a capacious reservoir, which is still further enlarged by the junction to its left side more anteriorly of the two common trunks of the principal middle left pulmonary veins.

"There is a deep notch lined by pleura, made

into the inner face of the great right lung, from before backwards, almost to its roots. The irregularly pyramidal slice of lung thus half-detached from, but still adhering by its base to the parent lobe, is the middle lung of quadrupeds. It is thrust upwards, and to the left of the loose posterior cava, but without forming the slightest adhesion to this vessel. In this situation, then, it would hang across the vein, were not a portion of its upper, or rather left surface parted up to the floor of the great reservoir just mentioned, and to some inches of the bevelled edge of the left lung, each preserving its proper pleura. The point of this little lung, with all its lower sides and angles, are free. This connexion between the middle lung, and the roots of the posterior pulmonary veins, is not the only one. Two, three, or more veins, coming from the left superior anterior angle of the middle lung, open their trumpet-shaped mouths into the floor of the reservoir precisely at the three points best calculated to pull it downwards, and to the right, when the middle lobe, filled by inspiration, is strained towards its parent lung, by the pleura

lining the notch. The anterior, the largest of these connecting veins, is inserted into the centre of the common trunk of the two middle veins already mentioned. The second, into the centre of the conflux of this trunk, with the great left posterior vein. The third (in the lung now before me) to the left of the centre of the conflux of this last with the right posterior veins. Thus, if the middle lung were pulled down from its adhesions to the left of the cava, and at the same time revolved upon its base, towards its parent lobe, its veins prolonged would form arcs of that angle, of which the right phrenic curtain, and the floor of the reservoir would represent the sides. When the horse's lungs are artificially inflated, the middle lobe makes precisely the movement described.

"In this arrangement there are the following remarkable circumstances.—1st. The principal veins of the left lung enter the right thorax.—2d. The veins of the middle lung cross the largest vein of the right lobe, to empty themselves into a particular point of the conflux of the left pulmonary veins.—3d. The veins of the middle

lung empty themselves at one of its extremities, instead of at its root. The purpose of this mechanism, I illustrated in the following manner:—"

Fourth Experiment.

"After having laid bare about half an inch of the lower surface of the left posterior pulmonary vein, I introduced into its cavity, towards the heart," (one end of a catheter,) "tying the vessel round it;" the other end "was immersed in a glass of red wine and water. By pulling gently upon the apex of the middle lung, in the direction in which it would move when inflated, the coloured liquid rose with such force, that it flowed abundantly into the reservoir. When I ceased to pull, the liquid ceased to flow; when I pulled the lung horizontally towards the left, the coloured water seemed rather inclined to return towards the glass; when I pulled horizontally towards the right, the liquid rose, but the more the middle lung was lifted from its attachments, the more rapidly the liquid flowed.

"The right posterior pulmonary vein, and

right side of the great reservoir," says Dr. Barry, "have no vein entering them from the middle lung, because the root of the posterior cava is extensively attached to them, a little farther forward.

"The diaphragm, in its retrograde descent, pulls upon the posterior cava in a direction downwards and backwards. The lower floor of the left, and the upper of the right sinus venosi, are thus removed from the axes of their respective cavities. The phrenic curtain pushed to the right, by the expansion of the middle lung, favors this movement of the cava, while it tends to widen its tube.

"The cavities to which this distending mechanism is applied, during inspiration, are exempt from pressure, whilst the pulmonary veins in direct communication with them, are exposed to the full pressure of the air rushing in by the trachea to distend the air-cells. Besides, the pressure of the atmosphere is exerted upon an extent of surface of the pulmonary veins, holding an inverse ratio of proportion to the capacity of their tubes.

"To comprehend the mechanism, by which the great pulmonary veins, or reservoirs of the left heart are expanded in man, it is only necessary to observe their connexion with the pericardium. A little tongue appears to be cut in this bag from behind forwards, to allow each vein to pass on to the heart, through a kind of square hole. This tongue is then pulled backwards and outwards, a little out of the general line of the insertion of the pericardium, and firmly glued to the anterior surface only of the vein. This mechanism is distinctly seen in man, as well on the right side as on the left, within the pericardium. There is a little pouch over each pulmonary vein, having its point directed outwards, whilst in quadrupeds no such contrivance exists. When the pericardium, therefore, in man, is brought forward by the elevation of the sternum, and when it is enlarged at its base by the expansion of the lungs, the anterior surfaces of the pulmonary veins, where they enter the left sinus venosus, must be strained forwards, whilst their posterior surfaces are retained in their place.

"If any further illustration were necessary of

the use of this peculiar attachment of the pericardium to the pulmonary veins in man, we have only to observe, that if the loose bag be pulled, however forcibly, in the direction which the movements of respiration give it, the strain will be brought on the anterior surfaces of the veins only, never on any portion of the arteries.

"This peculiar mode of connexion between the pericardium and pulmonary veins, does not exist in quadrupeds, except as far as concerns the anterior veins of the left lung, and even in these in a less remarkable manner; whilst the accretion of the contiguous sides of the cavæ and pulmonary veins, so marked, and so extensive in quadrupeds, is not found in man, at least not on the outside of the pericardium."

The laboured anatomical inquiry apparent throughout this quotation commands a degree of respect; at the same time it must be regretted, that so much pains should have been taken in the support of error, rather than in the developement of truth: for it is palpably evident, that Dr. Barry has not read impartially the anatomical language

of nature, but has had a particular object to accomplish, and has made a stretch of contrivance in taking this peculiar view of the mechanism of the pectoral viscera, in violation of the real laws under which it is governed; while others are brought to apply to circumstances pertaining to the dead, and not to the living and undisturbed state of the parts. For instance, in the last experiment of Dr. Barry, the animal was dead, (at least it is to be hoped so,) and if this were the case, many of the veins in all probability were empty: be this as it may, on opening the vein to introduce the tube, any blood which might be there would necessarily escape, and the vessel would collapse; therefore in raising the middle lobe of the lung to dilate the vein in question, there certainly would be what was intended, the formation of a vacuum in order to take a draft of the wine and water out of the glass: but as well might an automaton, that is made to play a game of chess, be deemed alive because he can perform the same action as a living being, as to conclude that a vein in its natural state, should circulate its blood by the same means as are made to accomplish this effect by art. The want of ana-

logy between the data of this experiment, and those pertaining to the vital function, which it was to represent, again argues the propriety of some of the remarks made in the Preface to this Analysis: for example, the pulmonary vein alluded to is known to be cylindrical, and to possess a tonicity, by which, to a certain extent, it can reduce its capacity to the volume of its contents; and as this vein is considered a reservoir, and as the circulation is constantly going on, it cannot become empty, or altered in its shape, under ordinary circumstances, although it may be in size. If then the distention of the lungs, drew further asunder the upper from the lower side of this vein, and thus elongated one of its axes, it would, on the contrary, shorten, according to a doubling ratio, the other at right angles with it, and so diminish its area, a disposition directly opposed to the formation of a vacuum.

The second point to be examined in this Experiment of Dr. Barry, is the mode in which atmospheric pressure was applied:—that upon the liquid in the glass was allowed to exert its entire force, and consequently would far exceed that

upon the pulmonary veins; while the air is pervading the lungs to occupy the general vacuum forming in the chest, which specially demands it, by consuming its direct force: but air, under these circumstances, moving in any one course with less velocity than would call into use its elasticity, would not be likely to press with any degree of force upon the sides of the bronchial tubes; and in viewing impartially the mechanism of the air passages, the cartilaginous structure of whose walls extend through a considerable series of their subramifications, it would appear that pressure upon the pulmonary veins was carefully avoided, unless upon the smaller branches. Again, since the veins for the most part, run parallel with these unyielding tubes, and as the air rushes at inspiration in a contrary direction to that which the blood in these veins takes, if any effect of the kind were to be produced, a *reflux* of the venous blood would be occasioned.

Could Dr. Barry have imagined any mode of explaining, that these pulmonary venous trunks, which he contends are reservoirs, became dilated during the *contraction* of the parieties of the

thorax, this part of his hypothesis would have born a rather more feasible aspect.

Enough has already been said of the improper substitution of incompressible tubes, to require any comment upon them in reference to this Experiment.

Although neither of the data upon which this Experiment was founded, obtain in the living animal, viz. the empty vein essential to the formation of the vacuum, the mode of applying atmospheric pressure, nor the incompressible tube; yet the intended result was produced, and proffered to the public, as a "well directed Experiment," illustrative of the pulmonary venous circulation.

It certainly ought not to have appeared in print: but since it has, it ought not to remain unnoticed, particularly as Experimental Lectures are at present given upon this theory, and attended most probably by some who would imbibe its errors, not yet being sufficiently acquainted with medical science to detect them. It should also be recollected, that Dr. Barry makes the following unmerited claim upon the public.

"The Experiments," says he, "therefore, that demonstrate this mechanism, and supply their important desiderata in physiology, must be entitled to the meed of novelty, along with whatever other merits they may possess."

Surely no one in the profession would refuse to admit the originality of these Experiments, nor ever think of putting in a prior claim.

From the examination of this fourth Experiment of Dr. Barry, we now proceed to review his original plan for forming the other vaccua, which he endeavours to prove connected with the cardiac extremities of the venous system pertaining to the left side of the heart. They are not allowed to be occasioned by the expansion of the thorax directly removing the atmospheric pressure from around these venous trunks, as supposed to be the case with the veins on the right side of the heart; but to depend upon the mechanical dilatation of the parts, or reservoirs, wherein they are said to occur. For instance, the vacuum in the last Experiment was accomplished by pulling the middle lobe of the lungs, as air would in distending it. Those about

to be considered, depend upon pleural attachments, connecting the parts respectively where they take place, with the parieties of the chest, viz. the sternum and diaphragm, in such a manner, that at each expansion of the thorax, the walls of these cavities are presumed to be drawn further asunder; thus forming a vacuum around the heart, by the sternum and diaphragm pulling in different directions the pericardium, and by these means, aided by the distension of the lungs, the sinus venosus of the left auricle of the heart, as well as the great pulmonary veins, are also dilated, producing the *desired* vacua; all of which may appear correct in idea, and display no little ingenuity on the part of Dr. Barry.

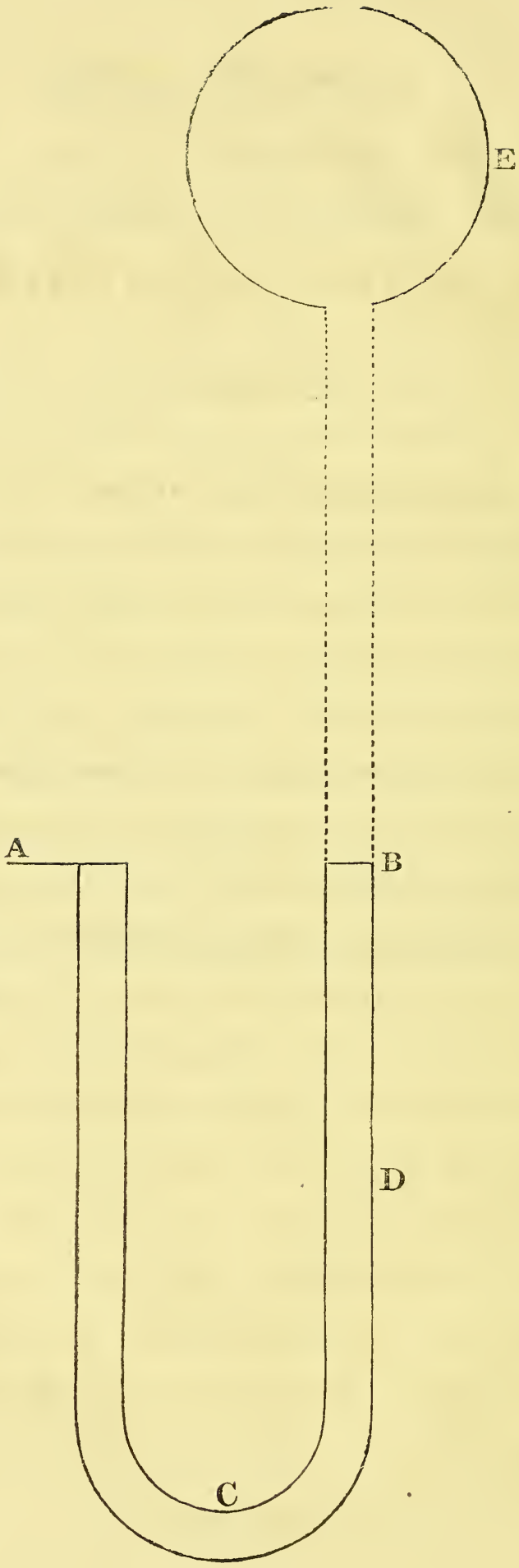
In objection to these premises it is urged, that the portions of the pleural membranes, which would be engaged in the purposes just described, are in the contracted state of the thorax comparatively flaccid, and would require a considerable enlargement of its cavity, to bring them into a degree of tension, that would be necessary to dilate these reservoirs; but if this enlargement of the chest during ordinary respira-

tion should not exceed one-sixth of an inch in either direction, these membranes could not possibly perform the agency Dr. Barry assigns to them; and further, that the Experiment I made (vidė page 37) by the removal of the parieties of the chest over the region of the heart of a rabbit, which survived seven hours without any impediment being evinced to the circulation of the blood, disproves at once that part of Dr. Barry's theory which rests upon the basis of vacua being formed within the sinus venosus of either side of the heart. This was a fact, which stands against any sophistry; and with regard to vacua taking place within the trunks of the pulmonary veins, it is clear that these reservoirs, as they are termed, can never be empty: therefore, the mathematical reason which has already been given, for their not admitting of dilatation, so as to produce any tendency to the formation of vacua, cannot, I should think, be invalidated by any Experiments or arguments.

Lastly, the presumed exposure of "the pulmonary veins in direct communication with their reservoirs, to the full pressure of the air rushing

in by the trachea, to distend the air-cells," has been before shewn to be completely chimerical and erroneous.

Dr. Barry having made the various Experiments he thought proper and necessary to the support of his theory, and having reasoned upon them, and deduced the conclusions, which have been already related, perceives that some further explanation is required to render the data consistent with its premises. To fulfil this purpose a Supplement is given, containing no less than twenty-four propositions in reference to an instrument. Of these, four or five are quoted, as applying the most strongly to the Experiments in question.


E
A
B
D
C

DR. BARRY'S SUPPLEMENT.

"To illustrate the physical application of the principles hitherto advanced to the circulation of the blood through the veins, and to shew that the two paramount laws of nature, gravitation and atmospheric pressure, are equally influential with regard to animated as to inert matter, we shall suppose two tubes of equal diameter, each in the form of the letter U.* Let each of the branches of these tubes be fifteen inches in length; one of these tubes shall be formed of a hard substance, such as glass, the other of a yielding distensible material, such as a vein.

"(1.) Let mercury be injected into the branch A of the first or hard tube, it will mount in the branch B, until both are full; and if the injection be continued, the mercury will flow out at B in jets equal to and synchronous with the injections at A."

* See Plate opposite.

The second proposition implies, that if mercury be injected into the compressible tube, not only would more be required to fill it from its distensible property, but that afterwards, the efflux would not be equal to the influx; "because, a part of the injecting force, and of the mercury injected, would be employed in producing fresh distension." There can be no objection to this reasoning, nor to that contained in the two following quotations.

"(3.) Let the branch B of the inflexible tube be prolonged to thirty inches, and let a vacuum be established in the reservoir E, with which this branch communicates: The mercury in the branches A and B will be forced by atmospheric pressure up to E. If the mercury can be removed from E, according as it arrives, without destroying the vacuum, all that is injected at A will flow into the reservoir E.

"(4.) Let us suppose the second or compressible tube under the circumstances just described, the portion B E being empty, its walls will be pressed flat by the weight of the atmosphere;

because the resistance which they offer is much less than that of the gravitation of the mercury, which, in this case, will not rise towards E, unless the tube be rendered incompressible by the introduction of another tube capable of resisting the pressure of the air, as in the first Experiment."

This last proposition directly applies to Dr. Barry's first Experiment, and is an admission that a compressible tube, as a vein, is not suited to become the medium of communication, through which a fluid, under the influence of atmospheric pressure, can be conveyed to a vacuum; and, therefore, that neither the first nor any other Experiment which Dr. Barry has made, similarly circumstanced, is calculated to form any sound proof of the correctness of his theory; unless some extraordinary method remain, such as no man of common understanding could, a priori, perceive, of qualifying this physical disagreement. An attempt of this kind is actually ventured on, in the next proposition of Dr. Barry, and since all his Experiments are in absolute need of such a

support, it becomes a very important one, and invites particular attention.

"(5.) If at the moment that the vacuum is formed, the flexible" (compressible) "tube be full *as far as E,* of a liquid ten or twelve times lighter than mercury," (meaning blood); "and if it be divided at short distances by valves, each forming a base to the column above it; and if the injections be continued not only at A, but at many other points between C and E; and lastly, if the sides of the flexible tube be attached to the parts about it, whose natural position tends to keep these sides asunder, then the tube B E will not be pressed flat, and the vacuum at E will act upon the contents of the whole tube, as soon as the injecting power shall have placed them within the sphere of its attraction."

In this proposition, it is unequivocally intended that its data should perfectly coincide with those of the circulation of the blood below the heart; thus the globe E is to represent a thorax, containing the right auricle of the heart, the thoracic

portion of the vena cava inferior, and also a ventricle. The flexible tube from E to B refers to the abdominal portion of the vena cava inferior; that from B to C to the ramified parts of the venous system; that from C to A to the arterial system, which must be imagined to extend up to the ventricle within E. If the reader will now *merely* recollect the *circumstance*, that there are valves dividing the venous system at short distances, and at the same time *forget* that they do *not* extend into the vena cava inferior, *nor* into the iliac veins, he will more readily conceive how a compressible can be brought to perform the purpose of an incompressible tube.

It is needless to make further comment upon this *new* datum, which Dr. Barry has brought forward, of the venous system being intersected at short distances by valves *as far as* the thorax (E), since it does not obtain in the animal frame; and, therefore, is not successfully opposed to the objections which he had anticipated respecting the incompressible tubes, and which have been repeatedly advanced in the progress of this review.

We will now examine the other essential parts of this proposition. It supposes that the vis a tergo actually propels the blood throughout the abdominal cava *as far as* the chest, that is to the very entrance of the reservoir, wherein the vacuum is to take place, and that it cannot extend its impulsive influence beyond, notwithstanding it is exerted in the direction towards it; while the atmosphere compressing at right angles with the line of motion which the fluid takes, can force it into the vacuum: This is a physical absurdity, which becomes the more glaring, when it is considered that the vis a tergo must frequently vary in its force two or three fold, according to the accidental state of the living being:—it therefore presumes that different degrees of power exert the same force, and that they all cease at the same point, viz. where they are least opposed, for the bare sake of Dr. Barry's theory.

It would appear, that when Dr. Barry comes to the rationale of his Experiments, the greatest discrepancy is manifested. He has by them contended, that the atmospheric pressure is the grand agent in the circulation of the venous

blood: and he now makes out that the vis a tergo impels this fluid close up to the vacuum, and where its influence could not possibly terminate, leaving, at the same time, scarcely anything for the atmospheric pressure to do; which is positively sweeping down at once the hypothesis which his various Experiments and Arguments have expressly endeavoured to raise.

It may be further objected, that the physical reasoning contained in this fifth proposition, does not hold good respecting the influence which atmospheric pressure would have upon the liquid in any part of this flexible tube, as from B to E. For instance, let it be divided by six valves into as many columns, it is plain that the atmospheric pressure would be the same upon each of them; consequently, the active pressure would only affect that column next to the vacuum: for if there were any intervening force allowed to operate between a more distant part of pressure and the vacuum, and that force were equal, one would be a counterpressure to the other; preserving the equilibrium, and thus destroying the effect of the pressure in regard

to the vacuum, and this reasoning applies to all the columns, excepting that contiguous to the reservoir; and Dr. Barry has merely asserted, but not attempted to show, why these valves (did they exist,) intersecting these columns, should destroy this equilibrium: It is clear that they would prevent the reflux of the blood; but it does not in the least follow, that they would alter the circumstances which are obviously opposed to suction. Should Dr. Barry make the same reply as he has done on a certain occasion, viz. that the vis a tergo is determining the blood in its proper direction, synchronously with the atmospheric pressure, it would be objected to on the ground, that however atmospheric pressure, and another important power, might act conjointly, the sum of their force would only be as their individual value combined; one could not increase the power of the other. But if it be proved that the atmosphere exerts no pressure by itself, it remains, that the vis a tergo alone is the efficient agent in the circulation of the venous blood.

It is not necessary to take further notice of

this long string of twenty-four propositions of Dr. Barry; they are evidently founded in supposition, not in experiment—They partially apply to the theory in question, illustrating little or nothing beyond a few minor points connected with gravitation.

Gravitation is the last of the three principal agents, which Dr. Barry considers essential to the circulation of the blood through the veins. Hitherto the examination has been confined to the two former powers, acting either in opposition to, or quite independently of, the influence of gravitation; which last will now form the conspicuous part of this stage of the enquiry. It is of course regulated by the positions of the body. In the recumbent, the heart can scarcely be said to be higher or lower than any other part of the frame, and therefore exempt from the agency of gravitation. In the erect position, the only parts placed above the thorax, excepting on some temporary and trifling occasions, are the head and neck; in these the blood is fully subjected to this power, while Atmospheric Pressure

can have but a limited effect upon it; that within the skull being secluded from its action. But it is decidedly proved, that the pressure of the atmosphere is not required to be exerted, in connexion with the vacuum forming in the chest, upon these parts of the body, by the familiar example of a horse under the operation of phlebotomy: wherein it is very common to extract the immense quantity of six quarts of blood, from the jugular vein, in about half an hour; the fluid issuing from the orifice in a full and rapid stream, while pressure is being made below, intercepting and destroying any possible influence, which the expansion of the thorax might have upon this part of the circulation. In this instance, the flow of blood is remarkably forcible, although quite independent of the *supreme* power of atmospheric pressure; and if gravitation ceases to operate by the recumbent position of the animal, the effect of the blood becomes slightly diminished, showing that the vis a tergo ought to be deemed the grand moving power.

Mr. Ellerby has paid particular attention to

the influence of gravitation upon the circulation of the blood, in reference to atmospheric pressure. He has made several experiments, which proved that the former was, although the least power according to Dr. Barry's views, superior to the latter. Upon the veins, both of the upper and lower extremities of different animals, Mr. Ellerby found that, on either occasion, as soon as the catheter was introduced a considerable way up the vessel, instead of the liquid in the cup being forced *up* by suction, the blood flowed *out* in a complete stream, evincing that gravitation was not in the least degree resisted by atmosperic pressure. In these instances the tube was no doubt pushed beyond the valves.

It is not to be supposed that Mr. Ellerby attempted to prove, that gravitation would be superior in its force to atmospheric pressure, under a fair barometical experiment; it was only upon the premises of Dr. Barry's hypothesis, that these were intended to exhibit this relation between these two powers. These experiments were in imitation of one of Dr. Barry's, differing only in passing the catheter to a greater distance up the

veins. They explain more simply and satisfactorily the want of success, for which Dr. Barry assigns other causes, less evident, in the following quotation at pages 14 and 15.

" Here, says he, it is essential to remark, that if the communicating tube be introduced into the femoral vein of a dog, or horse, *and pushed no further towards the heart,* inspiration will produce no effect upon the liquid in the cup; because the relative vacuum in the thorax can be filled up from the other veins of the animal's body, which will require a weight of atmospheric pressure to send forward their contents, less than would be necessary to force up the blue liquid, by the sum of all the secondary powers, such as contractility, vis a tergo, &c. The influence of Atmospheric Pressure invariably moves that first, which requires the least pressure."

The condition which Dr. Barry makes in this experiment, of pushing the tube " no further toward the heart," implies that he had performed it the other way; and his other experiments ex-

pressly require that the tube should be pushed "towards the heart as far as it would go." Making it still more probable, that Dr. Barry had also made the experiment in the same manner as Mr. Ellerby had done; if so, Dr. Barry ought to have related it, as the result would have militated against his theory, and would have saved him the unnecessary trouble of stating, "That the blood, which *runs contrary to its own gravity, arrives at the heart only during inspiration.*" And, "that the power which impels it at this moment through the veins, is *Atmospheric Pressure.*"

The Experiments of Mr. Ellerby were so conclusive, that I should think nothing further need be said, in special allusion to the effects of gravitation.

Dr. Barry, in the progress of his enquiry, has drawn several inferences from his Experiments, and from certain pathological phenomena, which have been reserved for investigation at the latter part of this Analysis. Dr. Barry relates a case, in behalf of the coincidence, which is occasionally to be seen between the swell and collapse of the

jugular veins, and the two stages of respiration, as being produced by Atmospheric Pressure. Instances of this nature are urged by Dr. Barry as strong proofs of the truth of his theory, and he refuses to admit the explanation which Haller gives of them, viz. " The causes of these phenomena, according to Haller, are, first, the greater facility afforded by the expansion of the lungs, during inspiration, to the passage of the blood through these organs. Secondly, the obstacles opposed to this same blood during expiration." Dr. Barry, on the contrary, thinks that expiration is not one of the causes of the swell of the jugular veins, but that the vacuum forming at the expansion of the thorax, calls into exertion the Atmospheric Pressure upon the veins, and occasions their collapse. And, in support of this opinion, he explains, in his fifth proposition, that the blood is constantly being propelled by the *secondary powers* into the veins, distending them close up to the thorax, until the vacuum tending to be formed, during *inspiration*, induces the atmosphere to exert an active pressure upon these vessels, that will force their blood into that cavity.

On all hands it is admitted, that the swell of the jugular veins occurs at inspiration, and that it is occasioned by the blood not being enabled to make its way into the thorax during that period. If the views either of Dr. Barry, or of Haller, were in themselves sufficiently explanatory of these phenomena, the swell and collapse of the veins ought always to be concomitant with respiration; but which is not the case. Therefore, in complete opposition to Dr. Barry's, and in the humble extension of Haller's mode of elucidation, the following is offered—That in health the blood arrives at the heart with due velocity, or force of motion, at the same time that respiration is performed, in that gentle manner, that at the contraction of the thorax the pectoral viscera are not compressed with a force that would impede the influx of their blood; consequently, that no accumulation would take place in the jugular veins, to constitute a swell. But, that when this just relation between these two different forces becomes destroyed, in favor of expiration, a resistance may be opposed to the ingress of the blood, and, in some cases, even

a reflux produced: As during the violent and convulsive action of coughing, sneezing, vehement speaking, and the like, a considerable distension is observed, not only in the jugular veins, but in others at more distant parts of the body, and these occurrences are sudden and temporary. On the other hand, there are instances of a less violent and more permanent nature, where both forces are in error. These cases involve great debility, with laborious respiration; in these, the circulation of the blood is so feeble, that this fluid enters the chest with less force than that with which the pectoral veins are compressed, during the contraction of the thorax, and in consequence of this resistance, it accumulates in the jugular veins, until the act of inhalation returns, when a freer passage is given to its entrance. Numerous examples of this kind are afforded in the latter stages of consumption, and of dropsy, and in all there is a laborious respiration. These phenomena must be received, I should think, as proofs that there is a slight barometical variation in the cavity of the thorax, during the different stages of respiration. The Experiments which have

already been related, upon the bellows and upon horses, also affirm its existence, as decidedly as any physical induction that could be inferred.

But although it be allowed, that there is a barometrical variation, or relative vacuum, in the cavity of the thorax during inspiration, it does not follow, as Dr. Barry presumes, "that all liquids in communication with an enlarging cavity, will be pressed towards it, if exposed at the same time to *Atmospheric Pressure;*" and on the premises that this is "an unalterable law." No one will dispute that the laws of nature are unalterable, but Dr. Barry, it would seem, has wrongly applied them, for it has been repeatedly shewn in this Analysis, that instead of atmospheric pressure being suited to become a responsible and effective agent to any vacuum, by impelling a fluid in communication through compressible tubes towards that cavity, it would become a barrier in the way, and most completely prevent the advancement of the fluid. Then, as the occasional collapse of the jugular veins at the expansion of the chest, makes it to appear, that the vacuum forcing, does influence

in some measure, the blood in its afflux towards it, it remains in evidence, that the other impulsive powers, must be answerable to the influence of this vacuum, viz. the vis a tergo principally, and the occasional one of gravitation. This was the view generally taken of this part of the circulation of the blood, prior to Dr. Barry's Memoir; but it was thought of too little importance, to be placed in a conspicuons light, and therefore scarcely had an existence. Its calculation of the relative powers of resistance which blood and air would meet in approaching the vacuum, which is forming at the expansion of the chest, may possibly prove useful in offering an idea of the degree of influence, which respiration might have upon the circulation of the blood.

One more point of consideration is, respecting Dr. Barry's supposition, that the Pressure of the Atmosphere would be actively applied to the venous system, external to the thorax, when in a state of distension by a fluid. It supposes that all the veins simultaneously suffer a collapse, or a certain degree of reduction, in their areas

at the moment of inspiration. Similarly to what is more generally, but perhaps erroneously thought, to occur in a less degree throughout the arterial system, coincident with the systole of the ventricle of the heart. This diminution in the capacity of the veins would be very considerable, about five or six times as much as in the arteries, agreeable to the difference between the frequency of pulsation and respiration respectively; notwithstanding this desparity, and another between the forced construction of the vein, and the spontaneous systole of the artery, the same method of reasoning will apply to both, as regards the movement of their contents: Then, let twelve inches of vein next to the thorax be supposed to be divided into twelve portions, and to contract uniformly at the same instant of time, diminishing its diameter throughout, equal to one. A question now arises; in what manner is the blood to accommodate its balance to this equal reduction in the size of the vessel? Suppose the twelfth, or most distant portion of the vein from the vacuum, becomes thus reduced in

its volume by the quantity of one. As condensation of the fluid is out of the question, and as it is moving forward, this superfluous quantity must be propelled into the eleventh portion: but this eleventh portion is synchronously sustaining the same reduction in its area, which requires, that distension of the vessel should also be out of the question: therefore, an increase in the velocity of the blood's motion, is the only mode of accommodation, and it will be found that, according to the premises of Dr. Barry, it must take place in these twelve portions, correspondently with the increasing series of one, two, three, four, &c. to twelve. For since each portion is to be contracted in its area; at the same moment it must urge on its individual superfluous quantity into its preceding portion: But it necessarily follows, that it must be receiving while it is giving: therefore, the successive transpositions of the respective quantities, will be in the following manner, viz. as the eleventh portion is diminishing its own by one quantity, while it is receiving one from the

twelfth; two quantities must become displaced in favour of the tenth, which is also to be deprived of one, while receiving two; requiring that three quantities should be transferred to the ninth portion, which, for the same reason, must propel four quantities into the eighth, and so on to the first portion, which would transmit twelve quantities into the thorax simultaneously, as the twelfth portion would be moving forward but one. It might be supposed also, that the Pressure requisite upon these successive parts, would also be greater as the velocity of their contents increased, to resist the stronger disposition to their distension.

In taking this limited view of the venous circulation, there appears many difficulties; and to extend it to all parts of the venous system, they would become vastly multiplied. We had therefore better apply for the "Wisdom" with which Dr. Barry has executed his "well directed Experiment, if we would be "contented with the exposition of what" (we might in our borrowed "wisdom)—suppose nature ought to do." Even then I fear we should not be contented

with the performance of the " mighty agency of the Atmospheric Pressure," in all cases of varicose veins.

The following quotation from page 35, further shows, that the physical arguments just brought forward are strictly applicable. Dr. Barry concludes, " 3dly. That as this power" (Atmospheric Pressure) can be applied to the blood of the veins only at the moment of inspiration, this blood must move with a velocity, which is to that of the blood moving through the arteries, as the time occupied by a whole respiration is to the time occupied, by a single inspiration.

The next conclusion of Dr. Barry is well deserving of notice, viz.

" 5thly. That as it makes no difference with regard to the event, whether the accumulation, which must be prepared for the expansion of the thorax, be made by two pulsations of the arteries, or by ten; it follows, that the frequency of the pulse cannot be taken as the measure of the velocity of the blood returning to the

heart, because it is the repetition of the inspirations which must regulate this velocity.

The above Inferences will be found in character, with most others which Dr. Barry has drawn, in respect to his Theory: for example, if the pulsations and respirations of a human being be taken as five to one, and at each inspiration a sufficient quantity of blood be drawn into the reservoirs of the thorax, to supply the right ventricle of the heart with this fluid, during five actions, it follows, that the right sinus venosus, and the veins leading into it, should possess the curiously combined powers of separating and sending off, in five equal portions, at regular intervals, a certain quantity of their contents, during each respiration, to distend the ventricle becoming the sole regulators of the heart and arteries; which latter organs must necessarily cease to pulsate during the period of inspiration: since these reservoirs could not possibly exert an active power of contraction, whilst they were passively distended:—respiration would, therefore, always controul pulsation; so that whatever the number

of respirations was in a minute, the same multiple of pulsations would respond; and that inflammation and other diseases could not affect the pulse, only as respiration became influenced by them, contrary to what is known to be the case.

Dr. Barry's Seventh Conclusion, &c. considered.

"That the lymph and chyle must be sucked up towards the chest, through the direct communications which the vessels peculiar to these fluids have with the subclavian and other veins. The question of absorption, therefore, which has hitherto puzzled physiologists so much, may now be considered as decided; for it is clear that the open mouth of a vein, or of any other vessel having the same kind of communication with the thoracic pumps, must absorb in direct proportion to the sucking powers applied to it, and to the pressure exercised upon the matter to be absorbed.

"If this last proposition be well founded, so ought to be the following corollary, viz.

"That the application of a powerful cupping-

glass to a recently poisoned wound, would prevent the absorption of the poisonous matter."

In this quotation Dr. Barry admits, that the Atmospheric Pressure is applied even to the smallest branches of the venous system. It now remains to be examined, how far the function of absoption is connected with Atmospheric Pressure. Dr. Barry has made a great number of experiments, proving, in a decided manner, the truth of this corollary. In some instances, the poison was conveyed under the skin of the animals operated upon, and allowed to exert its influence, until they were apparently upon the point of death; when the application of the cupping-glass over the wound, recalled them as it were to life.

However useful these Experiments may prove in a practical point of view; and although they may strongly infer, that the function of absorption is to a great extent dependant upon venous circulation, yet, they do not afford any proof that the venous circulation is itself dependant upon Atmospheric Pressure in connexion with a

vacuum. The application of a cupping-glass upon the soft parts of the body, will not, in general, destroy their Atmospheric Pressure more than a moment; for they will be necessarily forced up into the cavity, until the surrounding pressure become balanced by that within the glass; and this equilibrium may, perhaps, always be secured by the deeper seated arteries forcing their blood up into the more superficial veins; which are, at the same time, suffering powerful compression from the edge of the glass, and thus effectually detaining the blood which has become impregnated with the poison, by intercepting all possible communication with the columns of venous blood extending towards the heart. It is not, therefore, the absence of the pressure of the atmosphere upon the poisoned part, but the existence of a compressing force on that side of the wound, which intervenes it, and the heart, which prevents the poison having its usual deleterious effect upon the infected animal.

In support of this Conclusion, Mr. Ellerby's numerous Experiments upon the absorption of poison might be related. Mr. Ellerby by his own, fully acknowledged the success of Dr. Barry's

Experiments, at the same time most unequivocally annulling the inference Dr. Barry drew from them: viz. that the Pressure of the Atmosphere was the grand agent in propelling the blood through the veins towards the heart. Mr. Ellerby's Experiments were for the most part similar to Dr. Barry's; except in simply applying a ring of wood round the poisoned wound, instead of a cupping glass; but with a compressing force equal to that which would be derived from the rim of the glass. The results of these experiments were as pleasing as those of Dr. Barry's, although no attempt was even made to exclude the Pressure of the Atmosphere from the wounded parts.

These remarks are, of course, only referable to venous absorption. The following Experiments upon the bellows, will tend to prove, that Atmospheric Pressure has no control over the function of the lymphatics. A tube rather larger than the thoracic duct, was fixed by means of a cork, into one side of the bellows, in the place of one of the tubes which represented veins. When this apparatus was made to imitate the actions of the thorax, its movements were raised

above sixty in a minute, before any fluid could be drawn through this small tube, while less than forty would occasion the water to rush through the large pipes; showing that when different sized tubes are in communication with the same cavity, suffering a barometrical change below the medium of Atmospheric Pressure, that it requires a considerably less exertion of an impulsive power, to force water in a large volume, whose position will be proportionably small, into a vacuum, than will be found necessary to accomplish the same purpose by small pipes, which would offer a degree of friction immensely great; and when it is recollected how very trivial the quantity of fluid, which can be spared from the lymphatic system, is at each inspiration, it can be the more readily seen, how much easier it would be to increase the velocity of the blood passing through either of the veins, to the insenble degree, which would be necessary to make good the space which the few drops of lymph would have supplied.

An Analysis has now been attempted of all the Experiments and Arguments particularly deserv-

ing notice, which Dr. Barry has brought before the public, in support of his theory upon the motion of the blood in the veins; and it is with extreme regret that it is mentioned, that there has not been a single point found, which could be fairly considered of sound basis, upon which this hypothesis can rest, notwithstanding all the Experiments were successfully made; in as much as they produced the results desired by the experimentor, as proofs fit to be offered to the members of the medical profession. It is impossible to read Dr. Barry's book without being struck, either with the delusive plausibility of always hitting upon those Experiments and Anatomical Arguments, which merely bear the semblance of proofs; or with the extraordinary misfortune of missing upon all occasions the "well-directed Experiments:" especially as some of his own deviated from them but very slightly; but which, if they had happened to have been made, would very honorably have destroyed "the whole fabric of a baseless theory."

There are several facts and inferences, related by Dr. Barry, which have not been deemed

sufficiently important to require noticing; while there are many phenomena which he has made no allusion to, that might be brought forward efficiently opposed to his theory. For instance, the circulation of blood in the fœtus, without the aid of atmospheric pressure, is an instance so strictly analogous, that it would be no presumption to say, that it was unanswerable. A shark, fifty feet in length, possessed as it must be of an immense muscular power, yet not requiring the influence of a vacuum upon its venous blood, forcibly argues, that it is unreasonable to suppose, that respiratory animals which are so much less, should need this additional power to circulate their blood. Venesection, at any part of the body, if at the same time pressure be applied between the orifice and the thorax, becomes another disproof. The anatomical disagreement between the structure of veins, and that of the trachea, implies a difference in their mode of conducting fluids through them. Again, pulsation and respiration ought entirely to correspond according to Dr. Barry's premises. It has been objected by some gentleman, whose name I am not acquainted with, that in diving, the venous

reservoirs in the neighbourhood of the heart, ought to contain one or two hundred ounces of blood, to supply the arteries, during the long interval of respiration. I have frequently witnessed the circulation of the blood during ten or fifteen minutes in rabbits, whose thoraxes have been completely laid open, and this is a very common experiment.

It cannot be disputed, that Dr. Barry has invited the attention of the faculty to a mode of treating poisoned wounds, which had perhaps entirely fallen into oblivion. Although this be not a discovery, it is tantamount to one. Its practical utility cannot at present be estimated, nor will it be duly appreciated, until his theory on the circulation of the venous blood, has become silenced; for it has been so strenuously forced upon the minds of the faculty, that its assumed importance eclipses, in a measure, the great good which Dr. Barry is likely to occasion by his Experiments on poisoned wounds. It must be admitted, that Mr. Ellerby has made this practice more extensively useful, by having ascertained that pressure in any form, applied near the part,

so as to intercept the wound and the heart, will answer the same end as the cupping glass; for there is no inhabited part of the globe where a glass, a cup, or some kind of vessel, might not be promptly found, for the purpose of preventing any absorption of the deleterious matter, until the excision or destruction of the part could be made.

In Conclusion, I must be allowed to say, that the replies I have made to some of Dr. Barry's remarks in his Preface, and the views taken of his Experiments, are such as, in my humble opinion, are directly called for, and should not be passed by, for obvious reasons: At the same time, I beg to express a very high sense of Dr. Barry's merits, in having so ably and decidedly placed before the public, the practice which ought to be adopted in all cases of poison: and, if to save lives, is an important improvement in medicine, Dr. Barry has a right to claim an universal observance of this well-known motto—*Palmam qui meruit, ferat.*

FINIS.

Printed by J. Davy,
Queen Street, Seven Dials.